School's Out:
How Summer Youth Employment Programs Impact Academic Outcomes

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Abstract: Under the 2015 Every Student Succeeds Act, states have focused on reducing chronic absenteeism to close achievement gaps. We provide experimental evidence regarding the impact of Summer Youth Employment Programs (SYEPs) on secondary school outcomes. We find that the Boston SYEP increases attendance after participation, decreasing chronic absenteeism by 27 percent. We also present new evidence that SYEPs reduce dropout by 2.6 percentage points and raise graduation rates by 6.1 percentage points. These outcomes are correlated with increasing aspirations to attend college, gaining basic work habits, and improving social skills. A back-of-the-envelope calculation suggests a benefit-to-cost ratio of 4-to-1.

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Over the past several decades, many urban high schools have experienced little or no improvement in closing the academic achievement gap that exists along socioeconomic and racial lines (Musu-Gillette et. al. 2017; Duncan and Murnane 2011; Ladd 2012). Recently, chronic absenteeism among low-income and at-risk youth has been highlighted as a serious challenge for policies aimed at improving academic performance among these groups (Ready 2010, U.S. Department of Education 2016). In high poverty areas, as many as one third of all high-school students are chronically absent (Balfanz and Byrnes 2012, Sheldon and Epstein 2004) with greater rates of absenteeism among non-white students (U.S. DOE 2016). Chronic absenteeism has been linked to poor outcomes including inability to read at grade level, increased risk of drop-out (Mac Iver and Mac Iver 2010), and reduced rates of post-secondary enrollment (Balfanz and Byrnes 2012). Chronic absenteeism in even a single year between 8th and 12th grade is associated with a seven-fold increase in the likelihood of dropping out among public school students (Utah Education Policy Center 2012). In turn, high school dropout has been linked to poor outcomes later in life, from poverty and diminished health to involvement in the criminal justice system (Bjerk 2012).

Recognizing the importance of chronic absenteeism for student outcomes, 36 states plus the District of Columbia and Puerto Rico have chosen this metric as an accountability measure of "school quality or student success (SQSS)" under the Department of Education's 2015 Every Student Succeeds Act (Bauer et. al 2018). As a result, district leaders have been seeking a variety of ways to reduce chronic absenteeism, particularly among low-income youth. Recent experimental research examining the impact of other policy efforts aimed at boosting school attendance indicates that modest increases in attendance are possible using cash transfers/penalties (Dee 2011; Riccio et al. 2010) and early warning systems (Faria et al.

2017).Yet, these interventions have not achieved sufficient scale or scope to affect youth across multiple school districts or geographical areas (Jacob and Lovett 2017). For example, although a randomized controlled trial demonstrated that the Early Warning Intervention and Monitoring System (EWIMS) was shown to reduce the percentage of students with chronic absences and course failures, it was challenging for schools to implement with only two schools employing the full model and eight schools stopping the use of EWIMS during the study (Faria et. al. 2017).

This paper provides experimental evidence regarding the impact of large-scale Summer Youth Employment Programs (SYEP) on high school students' school attendance and academic performance. Despite the historically low unemployment rate, SYEPs continue to be important vehicles for employing youth in high-poverty and high-crime neighborhoods even as the economy has recovered from the Great Recession. With just under one-third of U.S. teens aged 16 to 19 years currently working, youth employment rates remain just shy of their pre-recession levels and are far below the 40 percent threshold that prevailed up until the 2000-01 recession (see Figure 1). Employment rates are even lower among non-white teens from low-income families living in high-poverty neighborhoods (Smeeding 2016; Sum et al. 2014). In addition, more than half of unemployed teens report that they are looking for their first job, suggesting that there may be fewer pathways for teens to enter the labor market—especially for those not enrolling in college (Dennett and Modestino 2013). Postsecondary credentials—whether it be a certificate, an associate degree, or a bachelor's degree—have become a requirement for many jobs that previously required only a high-school degree (Modestino, Shoag and Ballance 2019). At the same time, employer expectations for work readiness, communication, and other soft skills have risen—qualifications that are difficult for youth to demonstrate without a track record of work experience (Harrington et al. 2013). Together, these hurdles make it hard for many

young people, particularly those with weak school records, to enter the labor market and gain good work habits, including attendance.

Using a randomized lottery that provides access to the Boston SYEP, we evaluate the impact of the program on both short-term behavioral changes in skills and attitudes as well as longer-term school outcomes to better understand how these impacts are achieved and for whom the benefits are the greatest. This paper contributes to the existing evidence on the impacts of early work experience both in general and in terms of the specific experience provided by summer jobs programs. Prior studies of year-round workforce development programs aimed at youth and young adults have yielded mixed results. Often these earlier initiatives failed to improve outcomes without very high levels of investment, suggesting that other interventions could be more effective and efficient at achieving the same goals (Cave et al. 1993; Bloom et al. 1997; Schochet, Burghardt, and McConnell 2008; Millenky et al. 2011).

Yet, summer jobs programs differ from these earlier programs in several important ways. First, SYEPs primarily serve younger youth who are more likely to still be enrolled in school and less likely to have already held a job. As such, SYEP may act as a preventive measure compared to previous youth employment programs that were targeted at "opportunity" youth who had already dropped out of school and were struggling in the labor market. Second, the Boston SYEP incorporates several features—such as a formal career readiness curriculum, greater exposure to private sector employers, and job-skill ladders across summers—that are designed to specifically address skill deficits arising from a lack of opportunities among at-risk youth.

Finally, SYEPs occur in summer months when youth are often idle, creating fewer conflicts with their academic studies compared to year-round employment programs. Prior research shows that when students work too many hours, the likelihood of high school

graduation and college attendance decreases (Mortimer 2010; Stasz and Brewer 1999).[1] By providing steady income over the summer, SYEPs may reduce the number of hours a student needs to work during the school year and correspondingly increase the time devoted to academics.[2] In addition, SYEPs may help ameliorate summer learning loss among low-income and at-risk youth when school is out of session by providing the opportunity to practice existing skills or learn new skills on the job (Alexander, Olson, and Entwisle 2007; Cooper et al. 1996).

Understanding the mechanisms by which SYEPs can lead to better school outcomes down the road can help inform policymakers and practitioners about the types of interventions that might be successful at raising attendance or improving academic performance. In addition, these insights can help administrators improve existing programs and/or enable them to maximize resource allocation by targeting specific groups. We describe four primary channels through which summer jobs might affect secondary schooling outcomes:

(1) *Improving behaviors correlated with school success.* By placing youth in jobs that are supported by mentors and program staff, SYEPs help develop strong, supportive, and sustained relationships with adults and peers that are critical as youth move from adolescence into adulthood relationships (Nagaoka et al., 2015). In addition, the types of early work experience provided by SYEPs gives participants the opportunity to engage in tasks that help them develop the sense of agency, identity, and competency necessary for adult roles and success. Some SYEPs, including the Boston program, also offer programming aimed at improving non-cognitive skills such as responsibility, positive work habits, motivation, time

[1] Instead, the association between hours of work and school performance follows an inverted-U pattern, with students who work moderate hours performing at a higher level than students who work more or not at all (Stern and Briggs 2001).

[2] On the other hand, SYEPs could negatively affect educational outcomes if the work experience that students gain during the summer leads them to work more during the school year and focus less on school or forego education altogether in favor of employment. However, unlike private sector jobs, jobs obtained through the Boston SYEP are subsidized by the Mayor's Office of Workforce Development and end when the summer is over.

management, determination, self-confidence, and "grit"—attributes that have been shown to be important for adult success (Heckman 2008, Duckworth et al. 2007) and have the potential to boost attendance and reduce the likelihood of dropout (Jackson 2012).

(2) *Increasing career and academic aspirations.* Early work experience can also improve current job readiness skills as well as raise career and academic aspirations—both of which can lead to better long-term school outcomes, particularly for disadvantaged youth living in neighborhoods with few job opportunities. Indeed, mayors in cities such as Boston and Chicago are seeking to use SYEPs to provide meaningful employment experiences that can lead to alternative pathways for inner-city youth, recognizing that labor force attachment at an early stage in one's career typically predicts both higher employment rates and earnings later in life (Carr, Wright, and Brody 1996; Baum and Ruhm 2014). Greater exposure to employment also gives youth experiences that can shape their goals—whether it be to complete high school, obtain career training, or attend college (Lillydahl 1990; Mortimer 2010). The Boston SYEP curriculum also focuses on developing work-readiness skills such as exploring careers, writing a resume and cover letter, searching for jobs, completing online applications, and interviewing.

(3) *Reducing opportunities to engage in delinquent behavior.* Summer jobs programs may limit opportunities for youth to engage in delinquent activity or disrupt risky behaviors that may occur due to a lack of supervision or guardianship (Cohen and Felson 1979; Heller 2014; Modestino 2019). By providing youth with a set of socially productive activities, SYEPs may decrease the risk of exposure to, or participation in, delinquent behavior that could lead to truancy or other disciplinary actions affecting absenteeism such as suspension (Wilson 1996).

(4) *Providing direct income support to youth and their families.* Wages earned from employment in the program can help reduce poverty and provide resources that lead to better

school outcomes.[3] Indeed, based on our survey data, roughly half of youth participating in the Boston SYEP indicate that they help pay one or more household bills. In addition, by providing youth with a steady source of income, SYEPs may increase the motivation for youth to save for post-secondary education. The income channel may be particularly important for teens as employment rates for this population are typically lower and have been declining relative to that of other age groups, limiting their financial resources.

Although SYEPs have the potential to enhance youth outcomes along several dimensions, researchers have only recently focused on evaluating early work experiences provided by summer jobs programs. These studies typically use a randomized design to compare impacts for youth that were randomly selected into the program to youth that applied but were not selected. In terms of academic outcomes, the results are somewhat mixed but encouraging. For example, Leos-Urbel (2014) finds significant increases of one to two percent in school attendance for the treatment group relative to the control group during the year following participation in the New York City (NYC) SYEP, with larger improvements for students aged 16 years and older with prior low baseline attendance. Schwartz, Leos-Urbel, and Wiswall (2015) find small, but significant, increases in the share of NYC SYEP participants taking and passing statewide high school exams relative to the control group. However, other research indicates that the NYC SYCP did not have a positive effect on longer-term academic outcomes, such as graduating from high school (Valentine et al. 2017) or college enrollment (Gelber, Isen, and Kessler 2016).

More consistent SYEP impacts have been found in terms of criminal justice outcomes. Heller (2014) finds that participating in Chicago's One Summer Plus program decreased violent

[3] Note that it is often not possible to parse out any effect of the income associated with SYEPs from other changes related to the experience itself. Nonetheless, we lay out the main arguments supporting why we might expect SYEPs to improve outcomes independent of the income effect.

crime for youth in the treatment group by 43 percent relative to the control group, with much of the decline occurring during the 16 months after participation. Similarly, Modestino (2019) finds that the Boston SYEP reduced the number of arraignments for violent (-35 percent) and property (-29 percent) crimes among youth in the treatment group relative to the control group, with the effect accumulating during the 17 months after participation. Finally, Gelber, Isen, and Kessler (2014) show that participating in the NYC SYEP reduced the probability of incarceration and mortality from "external causes," including homicides, suicides, and accidents.

Several studies examine the link between SYEPs and subsequent employment and earnings but find little evidence of any permanent improvement that can be attributed to summer jobs programs. Two studies find that the New York City SYEP initially increases average earnings and the probability of employment, but the effects subsequently faded (Gelber, Isen, and Kessler 2014, Valentine et al. 2017). Another study using machine-learning to identify sub-group impacts in Chicago finds that employment improved only for participants that are more likely to be younger, enrolled in school, Hispanic, female, and less likely to have an arrest record (Davis and Heller 2017).

While the results of this research have demonstrated encouraging results in some cities— particularly for criminal justice outcomes— its utility for policymakers has been limited by the lack of insights into the *mechanisms* driving these improved outcomes. We build on this literature by linking survey data on changes in self-reported behaviors over the summer to administrative records on subsequent secondary school outcomes to explore channels discussed above and shed light on what works for whom, under what conditions, and why.

Overall, we find that the Boston SYEP had a significant and meaningful impact on improving attendance, reducing dropout, and increasing graduation rates among youth as well as

a small effect on academic performance. During the school year after participation, youth who were randomly selected into the SYEP treatment group experienced significant improvements in attendance rates of 1.9 percentage points, in part due to reducing their unexcused absences by 1.1 days. Moreover, youth in the treatment group were 7.8 percentage points more likely to achieve an attendance rate of 90 percent or better, reducing chronic absenteeism by 27 percent relative to baseline. Larger improvements are found for youth with initially low attendance rates and youth age 16 and older who are able to legally drop out. We also find small, but significant, impacts on overall GPA in the year after participation, but no meaningful improvements in standardized test-taking or scores. Second-year outcomes suggests that the program's impacts on attendance tend to fade out over time without a second summer of participation, although we cannot necessarily attribute a causal interpretation given that youth need to have applied for a second time, possibly indicating greater intrinsic motivation or ability.

Finally, we find that participating in the summer jobs program significantly reduces the likelihood of dropping out of high school and correspondingly raises the likelihood of graduating. Being randomly selected into the Boston SYEP reduces the likelihood of dropout by 2.6 percentage points—or 24.8 percent—relative to the control group. In addition, youth in the treatment group were 6.1 percentage points more likely to graduate from high school on time. Linking the academic records to self-reported survey data on short-term program impacts, we show that these outcomes are correlated with increasing aspirations to attend college, gaining basic work habits, and improving social skills over the course of the summer. Given that high school graduates have better outcomes than dropouts along a number of dimensions, including being more likely to be employed and earn a higher taxable income (Child Trends 2017) as well as being less likely to engage in criminal behavior or require social services (Lochner and

Moretti 2001), a back of the envelope calculation suggests that the long-term benefits of the Boston SYEP outweigh the costs by a factor of 4-to-1.

This paper is organized as follows. Section I provides an overview of the policy context and the experimental design. Section II describes the data and methodology that we use to evaluate program outcomes. Section III presents the estimates of the program's impact on both the longer-term secondary school outcomes as well as the short-term behavioral changes in skills and attitudes and analyzes the relationship between the two. Finally, Section IV concludes with a discussion of the policy implications and future research.

I. Background

A. *The Boston SYEP Intervention*

Introduced in the early 1980s, the Boston SYEP currently relies on approximately $10 million in city, state, and private funding to connect about 10,000 youth each summer with roughly 900 local employers. Participants work a maximum of 25 hours per week for a six-week period starting in early July through mid-August and are paid the Massachusetts minimum wage. Youth may be placed in either a subsidized position (e.g., with a local nonprofit, community-based organization, or city agency) or a job with a private-sector employer. In addition, the Boston SYEP provides 20 hours of job-readiness training using a hands-on, competency-based work-readiness curriculum. Modules include evaluating learning strengths, skills, and interests; developing soft skills such as communication, collaboration, and conflict resolution; and learning how to search for a job, draft a resume and cover letter, complete an online application, and answer typical interview questions.[4]

All Boston city residents aged 14 to 24 years are eligible for the program and youth apply

[4] The curriculum, Signal Success, was developed by the Commonwealth Corporation, a state agency, and is currently being piloted as part of the regular high school course offerings in both Lowell and Malden.

through one of the four intermediaries under contract with the Boston Mayor's Office of Workforce Development (OWD). The intermediaries are responsible for reviewing applications, matching applicants with jobs, supervising placements, and delivering the program's career-readiness curriculum. This analysis is restricted to youth who applied to the program for summer 2015 through Action for Boston Community Development (ABCD), a large and established nonprofit that works in all of Boston's 18 neighborhoods. Youth typically apply to the intermediary in their immediate neighborhood, and the program's administrative data indicate that 6.8 percent apply to more than one agency—although none receive more than one offer of employment. However, because of the ability for youth in the control group to obtain an SYEP placement through another intermediary, we do control for this explicit cross-contamination, although the adjustment has little impact on our results.[5]

B. *Experimental Design*

We focus on ABCD because it is one of the two intermediaries that make use of random assignment due to the high number of applications it receives for the limited number of SYEP jobs that are available.[6] The enrollment period typically spans February through June, and applicants are notified of their lottery status and job assignment in late June. ABCD uses a computerized system with a random-assignment algorithm to select youth based on their applicant ID numbers and the number of available slots which is determined by the amount of funding each year. This system effectively assigns the offer to participate in the program at random, creating a control group of youth who apply to the SYEP but are not chosen. Of the

[5] Administrative data provided by the City of Boston shows that only 3.0 percent of the control group obtained a job through one of the three other summer job intermediaries.

[6] The other intermediary that uses random assignment, the Department of Youth Employment and Engagement (DYEE), does so only on a partial basis where 60 percent of the jobs for a given employer are assigned randomly and the other 40 percent are selected by the employer. In addition, DYEE chose not to implement the survey during the summer of 2015 so it is not possible to test program mechanisms.

4,235 youth who applied to ABCD in 2015, a total of 1,186 (or 28 percent) were offered a job via random assignment, leaving 3,049 individuals in the control group. Of those selected by the lottery, 83.6 percent accepted a job offer, with only a handful dropping out during the program.

Table 1 provides descriptive statistics for the preexisting characteristics of SYEP lottery applicants collected by ABCD, which reflects a predominately low-income, school-aged population.[7] On average, approximately 88 percent of applicants were in school at the time they applied, with a mean age just shy of 16 years. A slightly higher percentage of applicants were female, and just over 50 percent were African American. Although over 95 percent indicated that their preferred language was English, roughly 7 percent identified as having limited English ability. In addition, nearly 7 percent reported being homeless and upwards of 18 percent acknowledged receiving cash public assistance of some form.[8] Less than 5 percent listed themselves as having a disability.

Based on these observable characteristics, the youth selected by the ABCD lottery appear to be almost identical to those not selected, confirming that the lottery is indeed random. The one statistically significant difference is the share of Asian youth being slightly higher (7 percent) in the treatment group versus the control group (5 percent) (see Table 1). We note that having at least one statistically significant difference at the $p<0.10$ level would be expected by random chance when testing 15 different characteristics. The sample is similarly balanced among the

[7] Table A2 in the online appendix shows that ABCD draws applicants from all 18 Boston neighborhoods with greater representation among those with higher shares of youth age 0-17 (see Figure A2). Approximately 80 percent of ABCD applicants are Boston Public School (BPS) students—similar to the proportion of Boston high school-aged residents that are enrolled in BPS (Boston Foundation, 2006). Finally, ABCD applicants have similar gender and racial characteristics in comparison to the population of low-income Boston youth (see Table A3).

[8] Cash public assistance includes Emergency Assistance to Elderly Disabled and Children, Social Security Income, Social Security Disability Income, Temporary Aid to Families with Dependent Children, Unemployment Insurance, or worker's compensation.

school-aged population.[9]

To provide some indication as to whether the Boston SYEP provides a meaningful intervention in terms of employment, Figure 2 displays descriptive information about the self-reported summer employment experiences among individuals responding to an end-of-summer survey of both the treatment group and control groups. Note that only 26.4 percent of those in the control group responding to the survey had worked during the summer, indicating their comparative inability to secure jobs even with Boston's relatively low unemployment rate of 4.4 percent in July 2015.[10] Survey respondents indicated that youth in the control group who found a job worked fewer hours per week than SYEP participants (see panel A), but had more variation in the types of daily work they did; in comparison, over half of SYEP participants worked at a day care or day camp (see panel B). Yet, SYEP participants were more likely than their counterparts in the control group to report that they would consider a career in the type of work they did, had an adult they considered a mentor and who they could use as a reference in the future, and felt better prepared to enter a new job (see panel C). Although self-reported, these experiences suggest that the Boston SYEP provided a meaningful intervention in terms of the likelihood, intensity, and type of employment obtained. The next question is whether the Boston SYEP had any meaningful positive impacts on secondary school outcomes for youth.

II. Data and Empirical Methodology

Previous studies of early work experience have been skeptical of empirical findings, citing positive selection into employment based on the preexisting characteristics of teens who

[9] We test for balance using separate models estimating the effect of winning the lottery on preexisting applicant characteristics among school-aged youth for gender/race groupings (see Table A1).

[10] Quarterly wage record data provided by the Massachusetts Division of Unemployment Assistance recorded a similar proportion of youth in the control group (28.2 percent) as having worked during the third quarter (July-September) of 2015.

work versus those who do not (Hotz et al. 2002; Bacolod and Hotz 2006). To address this potential bias, we rely on a lottery assignment that effectively controls for selection into the program while also accounting for changes that might occur during the normal course of adolescent development. The first phase of the analysis uses administrative data during the one to two school years following the intervention (2015-16 and 2016-17) to assess SYEP impacts on longer-term secondary school outcomes. The second phase of the analysis uses survey data on self-reported behavioral changes in skills and attitudes that occur during the summer to provide insight into program mechanisms that may have enabled participating youth to increase their attendance and/or academic performance.

While some observers question whether a six-week intervention can provide a meaningful turning point to affect youth life-course development, such impacts may be greater for at-risk youth (Sampson and Laub 2003). As one researcher concluded, "Having a positive work experience can help to turn you around. For those who have a lot of disadvantages, any positive experience is likely to have a greater impact than on people with a lot of advantages already" (Mortimer 2010, p. 8-11). This may be especially important for teens growing up in low-income neighborhoods with failing schools (Chetty, Hendren, and Katz 2016). As such, we also test for heterogeneous impacts where one might expect to see a disproportionate impact based on a greater likelihood of chronic absenteeism—specifically among older youth, males, those with limited English skills, at-risk youth defined as receiving public assistance, and students with baseline attendance rates that indicate chronic absenteeism (Utah Education Policy Center 2012).

A. *Using Administrative Data to Assess SYEP Impacts on School Outcomes*

Data for the first phase of the analysis come from school records obtained from the

Massachusetts Department of Elementary and Secondary Education (DESE), which provide information on all students within the state of Massachusetts, including both private and public schools. This rich data source contains information on secondary school outcomes including attendance, course grades, statewide test scores, dropouts, and high school graduation. The benefit of using administrative data is that one avoids the problems of self-reported data such as social desirability bias, which might be large if individuals in the treatment group feel compelled to embellish their school performance when applying for a summer job.

The drawback to administrative data is that individuals must be matched across two different record keeping systems, often resulting in a less than perfect match. Since the individual-level SYEP and DESE files do not share a unique common student identifier, students were matched based on their name and birth date. Of the original sample, 79.6 percent were in school and in grades 8-11 during the 2014-15 school year before applying to the summer jobs program and would be expected to attend school during the year after participating. Of these, almost all (96.9 percent) were matched to the 2014-15 DESE file—a much higher match rate than that of previous summer jobs studies, likely due to having state-level records that capture youth in both public and private schools, even if they switch schools within the state.[11]

Even though the lottery appears to be random and the match rate with the administrative data is quite high, estimates of the impact of SYEP on student outcomes could be biased if there is selective attrition from enrolling in school during the year following participation in the program. Of the students in grades 8 to 11 in the school year prior to SYEP, 90.4 percent of those selected by the lottery were enrolled in the following school year compared to 91.1 percent of

[11] Leos-Urbel (2014) reports a 77 percent match rate for applicants to the New York City summer jobs program. He attributes this lower match rate to unmatched records including an unknown number of students in private or parochial schools or schools outside of New York City, as well as nonstudents.

those not selected, confirming that selective attrition is not a problem.[12] To more rigorously test for selective attrition, Table 2 presents estimates of the effect of winning the lottery on the same preexisting demographic characteristics as before. The first column limits the sample to youth who were matched in the 2014-15 school year and the second further constrains the sample to those who were also enrolled in the 2015-16 school year. The SYEP indicator does not significantly predict any individual characteristics—with the exception of the one characteristic (e.g. Asian) that was noted in the earlier balance test for the full sample—suggesting that overall SYEP lottery winners and losers did not differentially attrit.[13] To further test for validity and balance, we also estimate the effect of the lottery indicator on individual baseline outcomes, where possible, and find no significant pre-existing differences between youth in the treatment versus control groups as would be expected under random assignment.[14]

To assess the impact of the Boston SYEP, we compare school outcomes during the period following the intervention for the treatment versus the control group. Because SYEP participation is allocated via lottery, we obtain causal estimates using a simple comparison of means on the outcome of interest. Specifically, we compare outcomes for youth offered an SYEP placement (the treatment group) to those not offered a placement (control group). This "Intent to Treat" (ITT) estimate measures the impact of *offering* the program on the outcome. In many cases, this is the policy relevant estimate for program administrators who want to account for take-up among the applicants, rather than just assessing outcomes for those who also choose to participate. Nonetheless, because not all youth accept the offer, the ITT estimate will understate

[12] These attrition rates are similar to those of prior studies such as Leos-Urbel (2014) which reports that 93.5 percent of those selected by the NYC lottery were enrolled in the following school year compared to 93.4 percent of those not selected. See Table A.4 in the online appendix for these tabulations.
[13] We also find no evidence of attrition by grade level (see Table A5).
[14] Note that it is not possible to test for baseline outcomes for taking the MCAS or for high school graduation. See Table A6 in the appendix for these comparisons.

the effects of the program for those youth who choose to participate. As such, we also provide treatment-on-the-treated (TOT) estimates using a two-stage-least-squares method.

We measure multiple outcomes of interest during the post-intervention period within each domain: attendance, course performance, standardized test taking and scores, dropout, and high school graduation. The construction of these variables is described in detail in the online appendix. Note that although covariates are not necessary to derive unbiased impact estimates when treatment is randomly assigned (Bloom 2006), we also use the following regression framework to control for individual characteristics and improve the precision of our estimates:

$$Y_{it} = SYEP_i \pi_1 + X_{i(t-1)} \beta_1 + s + \mu_{it1} \quad (1)$$

where Y_{it} is the school outcome, $SYEP_i$ is a dummy variable indicating the individual received an offer to participate, $X_{i(t-1)}$ is a set of pre-existing demographic characteristics, academic characteristics, and baseline school outcomes[15], s is a vector of school fixed effects to control for the influence of time-invariant school characteristics on educational outcomes, and μ_{it1} is a stochastic error term. Robust standard errors are clustered at the student level. We use both OLS as well as alternative nonlinear methods to relax the linear functional form assumption.[16]

Additionally, we are interested in exploring whether SYEP impacts fade over time as well as if additional summers (e.g., increased "dosage") enhances outcomes. Given that the program is oversubscribed, understanding the dynamic nature of program impacts can help

[15] Demographic characteristics include age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status. Academic characteristics include indicators for grade, enrollment in the Boston Public School district, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years. The inclusion of these controls does little to affect the point estimates but does improve the precision.

[16] To analyze differences in the number of incidents—a count variable—we use a Poisson quasi maximum likelihood estimator (QMLE). The consistency of this estimator only requires the correct specification of the conditional mean, not the entire distribution. To analyze differences in the likelihood of an event, we use a probit estimator. Marginal effects are reported in all tables when using these nonlinear estimation methods.

policymakers better allocate scarce resources to achieve meaningful outcomes while serving as many youth as possible. To explore these questions, we make use of an additional year of DESE data for the 2016-17 school year that provides information on school outcomes for the second academic year after participating for the summer 2015 cohort. We then use administrative program data from OWD to identify youth who participated in the program during the summer of 2016 to construct indicators for whether youth had participated for only one summer (SYEP1) or two summers (SYEP2).[17] About one-quarter (26.8 percent) of youth in the original treatment group participated for a second summer, yielding enough variation to assess the importance of both dosage and fade out. To estimate separate impacts by number of summers of treatment, we use equation (2):

$$Y_{it} = SYEP1_i \pi_{10} + SYEP2_i \pi_{11} + X_{i(t-1)} \beta_1 + s + \mu_{it1} \quad (2)$$

Note that there are some limitations to this analysis. For example, having won the lottery in the first year is likely to increase the likelihood of applying for a second time and the opposite is likely to be true for those who did not win the lottery the first time. Indeed, only 3.7 percent of those in the control group end up participating in the program during the summer of 2016. As such, our estimates of the impact of a second summer of participation (π_{11}) primarily reflect the impact of the program conditional on having won the lottery the first time. Nonetheless, we believe it is still informative to explore program impacts two years post participation and assess how much can be explained by the number of summers of participation.

B. *Using Survey Data to Explore SYEP Program Mechanisms*

To explore program mechanisms, we link the secondary school outcomes described above to the short-term behavioral changes in skills and attitudes observed during the summer

[17] Note that youth who participated for only one summer includes both members of the original treatment group who only participated in summer 2015 as well as members of the control group who participated in summer 2016.

for the treatment group, as measured by a pre-/post-program survey. The survey was originally developed by the Boston Youth Violence Prevention Collaborative to measure individual behaviors correlated with youth violence. We built on this original framework to expand the survey's content, adding questions related to job readiness as well as postsecondary aspirations. Whereas the first part of the analysis using administrative data establishes the causal impacts of the Boston SYEP on school outcomes, the goal here is to provide a glimpse into *how* the program achieves these outcomes. Because we rely on self-reported survey data to assess the short-term behavioral changes in skills and attitudes, this second part of the analysis should be regarded as more exploratory in nature.

1. *Assessing Short-Term Behavioral Impacts*

To explore how the Boston SYEP affects youth behavior over the course of the summer, ideally one would want to compare the change over time in the pre/post-program survey results for the treatment versus the control group. However, while the survey was administered to participants at both the beginning and the end of the summer to assess changes over time, program administrators chose to administer the survey to the control group only at the end of the summer to provide a point of comparison. Therefore, we measure program impacts as those outcomes where there was a significant improvement among participants over the summer as well as a significant difference relative to the control group at the end of the summer.

There are several potential sources of bias arising from this analysis. First, it might be the case that the individuals in the treatment group who responded to the survey differ from those who did not. Fortunately, the high response rate among the treatment group (66.9 percent, N=663) was sufficient such that there were no significant differences in observable characteristics for the entire treatment group versus those responding to both the pre- and post-

survey.[18] Thus, short-term behavioral changes in skills and attitudes measured over the course of the summer for the treatment group are likely to be unbiased.

A second source of bias could arise from the differential response rates of the treatment and control groups. Indeed, while the number of respondents in the control group was similar (N=664), this represented a response rate of only 21.8 percent. Even so, the two groups were randomly selected, so we can use the observable characteristics to determine the direction of bias. Relative to the treatment group, respondents from the control group were more likely to be older, female, identify as white or Asian, and indicate that they live in a two-parent household.[19] We argue that the selection bias goes *against* finding an impact for the Boston SYEP, given that the survey respondents in the control group exhibit characteristics that are on average associated with *better* outcomes.[20] Nonetheless, to minimize selection bias due to survey response rates, we also control for observable characteristics using equation (3):

$$M_{it} = SYEP_{it}\,\pi_2 + X_{it}\,\beta_2 + \mu_{it2} \qquad (3)$$

where M_{it} is one of the short-term program outcomes (e.g., social skills), $SYEP_i$ is a dummy variable indicating the individual received an offer to participate, and X_{it} is a set of demographic characteristics collected at the time of the survey. Because the selection among survey respondents in the control group is correlated with better outcomes, the coefficient π_2 is likely to provide downward-biased estimates of the program's impact on short-term behavioral

[18] Table A7 in the online appendix compares the characteristics of the full treatment group to those participants who responded to the survey.
[19] Table A8 in the online appendix compares the characteristics of the survey respondents across the treatment and control groups.
[20] In terms of academic outcomes, females are more likely than males to graduate high school and attend college (Autor and Wasserman 2013, Hugo-Lopez and Gonzalez-Barrera, 2014). In addition, standardized test scores are lower among African-American children and those living in single parent households (Jencks and Phillips, 1998). Higher employment rates are observed among females, whites, and older youth (Child Trends, 2017). Age, male gender, and living in a single-parent home have been shown to be significant predictors of re-offending among youth (Cottle et. al., 2001).

outcomes. We also recognize that self-reported data is subject to measurement error arising from social desirability bias and item non-response (Meyer, Mok, and Sullivan 2015). However, if we assume that measurement error is random across the treatment and control groups, this would reduce efficiency but not cause bias. Indeed, the item non-response rate for the survey questions used in the analysis was less than 5 percent for both the treatment and control groups.[21]

2. *Linking Short-Term Behavioral Impacts to Academic Outcomes*

Ideally, a full mediation analysis would be used to generate evidence for how the Boston SYEP program improves academic outcomes (Baron and Kenny 1986, Keele et al. 2015). However, because the post-survey was administered to the control group anonymously rather than confidentially, as was done for the treatment group, we can only link the survey responses to the school record data for youth in the treatment group who responded to the survey, ruling out a full mediation analysis. Nevertheless, it is still possible to explore whether improvements in the short-term behavioral impacts on skills and attitudes are correlated with better school outcomes to shed light on the program's mechanisms. To do this, we modify equation (1) as follows:

$$Y_{it} = SYEP_i\,\pi_3 + X_{i(t-1)}\,\beta_3 + s + \Delta M_i\,\delta + \mu_{it3} \quad (4)$$

On the left-hand side, the dependent variable is one of the longer-term school outcomes (e.g., attendance rate) while on the right-hand side is a dummy indicating positive improvement for a specific short-term behavioral impact ΔM_i (e.g., being on-time). A positive and significant coefficient on ΔM_i indicates that improvement in the short-term behavioral impact observed during the summer of participation is positively correlated with the subsequent improvement in school outcomes, such as attendance. Moreover, if the coefficient on the $SYEP_i$ dummy in

[21] Table A9 in the online appendix compares the non-response rates across the treatment and control group for each question of interest from the post-survey.

equation (3) is smaller in magnitude than that in equation (1), this would suggest that ΔM_i plays a role in improving attendance separate from simply being assigned to treatment. Given that this approach could also be driven by unobservable characteristics such as youth motivation (e.g. as reflected in their willingness to answer the survey), we also test whether these same relationships hold when the sample is restricted to participants completing both the pre- and post-survey.

Note that the mediator analysis implicitly assumes that there was no change in the short-term behavioral measures for youth in the control group. We argue that this assumption is plausible if the analysis is restricted to those short-term program impacts for which there was both significant improvement over time among participants and for which the gains were significant relative to the control group at the end of the summer. Moreover, there is abundant evidence that youth typically lose academic and social skills and experience a decrease in college aspirations over the summer, and this tendency is particularly acute among disadvantaged groups (Cooper et al. 1996; Panayiotou et al. 2017; Castleman and Page 2014).

III. Results

A. *Assessing SYEP Impacts on Academic Outcomes Using Administrative Data*

1. Attendance

In terms of attendance, we find that the Boston SYEP had strong positive impacts across all of our measures, including chronic absenteeism, during the first year after participation. Table 3 reports the ITT estimates of the difference between the treatment group and the control group from equation (1) on several attendance outcomes with each successive column adding an additional set of controls. The first column shows the raw difference with no controls and indicates that the attendance rate in the year following the summer jobs program was 2.5 percentage points higher for students in the treatment versus the control group. Adding in

individual controls for participating in SYEP through another intermediary, demographic and academic characteristics, and baseline outcomes have little impact on the estimate. The inclusion of school fixed effects reduces the magnitude of the coefficient somewhat, perhaps reflecting different attendance policies or cultures across schools. With the inclusion of all controls we find that attendance rates improved by 1.9 percentage points or 3 school days during the year after participation and are similar in magnitude to those of Leos-Urbel (2014).

More importantly, the magnitude of the program's impact on attendance was large enough to have a meaningful impact on chronic absenteeism. Controlling for all individual and school factors, the treatment group was 7.8 percentage points more likely than the control group to have attended at least 90 percent of the school year—below which is considered chronically absent. This reduces chronic absenteeism by 27.2 percent relative to the baseline and is similar in magnitude to impacts attributed to other initiatives focused on boosting attendance such as the Early Warning Intervention and Monitoring System (EWIMS).[22] Interestingly, the relative difference in attendance rates between the treatment and control groups in the post-period is largely driven by the treatment group *not* experiencing a decrease in their attendance rate from the prior year. Given that attendance typically falls as youth age, due to less adult supervision and rising rates of delinquency, this finding suggests that the SYEP might operate as a preventive intervention for chronic attendance among school-aged youth.

Indeed, the relative improvement in attendance among the treatment group did not simply reflect fewer days out due to illness or other excused absences, but also a reduction in truancy,

[22] A recent RCT of the Early Warning Intervention and Monitoring System (EWIMS) indicate that the program has reduced chronic absenteeism rates from 14 to 10 percent—an improvement of 28.6 percent relative to baseline. EWIMS is primarily a monitoring system, rather than a single intervention, but includes highly detailed and structured guidance for schools, along with a tool to help monitor student attendance and academic performance. Interventions for students found to be off-track are determined and implemented by school or district staff. See https://ies.ed.gov/ncee/edlabs/regions/midwest/pdf/REL_2017272.pdf for more details.

suggesting a behavioral shift in the propensity to attend school.[23] Average days attended increased by 3.1 days among the treatment group compared to the control group and this was partly driven by a reduction of 1.2 days of unexcused absence. This reduction in truancy represented a 10 percent decreased in unexcused absences relative to the baseline and is on par with other interventions aimed at addressing chronic absenteeism, such as notifying parents of absences via postcard (10 percent) or text messaging (17 percent).[24]

Looking at the two-year impacts suggests that the program's effect on attendance tended to fade out over time without a second summer of participation. Although all of the coefficients reflect continued improvements into the second year, they are by and large not statistically significant for youth participating for only one summer. The only exception is that those participating for only one summer were significantly less likely to see their attendance rates decrease from the prior year compared to the control group. In contrast, youth that are randomly selected to participate for a second summer appear to maintain the 1.9 percent improvement in their attendance rate from the first year, due to an additional 4.7 days attended, including a significant reduction of 2.8 days of unexcused absences.

2. Course performance and standardized testing

In terms of course performance, we find that the program had a small impact on overall GPA and course failures in year one that grow over time with a second year of participation. In terms of the one-year outcomes, Table 4 shows that when controlling for all individual and school factors, the treatment group had overall GPAs that were 0.08 points higher than the

[23] This is consistent with prior research by Heller (2014) and Modestino (2019) that shows SYEPs reduce delinquent behavior as captured by criminal arrest and arraignment data.

[24] Rogers and Feller (2014) randomly assign parents of high-risk, K-12 students to receiving received one of three yearlong regimes of personalized information. The most effective regime reduced chronic absenteeism by 10 percent across all grade-levels, partly by correcting parents' biased beliefs about their students' total absences.
Bergman and Chan (2017) find that low-cost text messaging to parents has been shown to improve attendance by 17 percent.

control group, although the impact was only marginally significant. Similarly, we find a small reduction in the likelihood of failing a course during the first year after participation but it is not statistically significant, except when controlling for school fixed effects.

In contrast, the second year impacts on course performance are larger in magnitude and significance—but only for youth who participated for two summers. Table 4 indicates that the overall GPA of the treatment group was 0.12 points higher (an improvement of 6.1 percent from baseline) and the likelihood of failing a course was reduced by 6.1 percentage points. More striking was the 10.2 percentage point reduction in the likelihood of failing an ELA course during the second year. Overall, these results suggest that the impact of the program on academic performance is less immediate than that of attendance and may accumulate over time with continued participation in the program. However, we need to be careful in attributing a causal interpretation to the second-year results for the repeat participants given that youth need to have applied for a second time, possibly indicating greater intrinsic motivation or ability.

We also explore whether participating in the Boston SYEP had a measurable impact on student performance on the Massachusetts Comprehensive Assessment System (MCAS), a statewide standardized test. Students must receive a passing grade on both the mathematics and ELA tests to receive a high school diploma.[25] Similar to Leos-Urbel (2014), we find no impact on performance in terms of improving scores or raising the likelihood of proficiency (see Table 5). In contrast, Schwartz et al. (2014) find a small, marginally significant increase in passing any New York State Regents exam, as well as in the number of exams passed, for SYEP lottery winners in New York City. These two prior studies also found an increase in the likelihood of

[25] Note that because students take the MCAS in the 10th grade, we must observe participants as 9th graders in the prior summer to assess whether the program has any impact on test-taking or performance, limiting that number of students for whom we can assess MCAS impacts.

taking standardized tests. Yet, we find little increase in the likelihood of taking the MCAS, possibly because—unlike the Regents exams—the MCAS is a mandatory requirement for high school graduation. Nonetheless, we do find a small increase of 3.6 percentage points in the likelihood of taking the ELA MCAS on time, but the effect is only marginally significant.[26]

3. Dropout and high school graduation

While improving attendance rates and course performance are worthy goals in and of themselves, the primary interest in reducing chronic absenteeism is to prevent dropout and increase the likelihood of high school graduation. Figure 3 plots dropout and high school graduation rates for the treatment and control groups for the first year after participating in the Boston SYEP as well at any point after having participated in the program.[27] Controlling for all individual and school factors, we find that participating in the summer jobs program significantly reduces the likelihood of dropping out of high school by 1.4 percentage points in the year after participation and by 2.6 percentage points during the remainder of one's high school career. Correspondingly, participating in the Boston SYEP raises the likelihood of graduating from high school on time by 6.1 percentage points and of graduating at any point after participating in the program by 5.8 percentage points.

4. Heterogeneity in outcomes by subgroup

As prior research has shown, it could be the case that the impact of the Boston SYEP on school outcomes is greater for more marginal students (Leos-Urbel 2014). As such, it is natural

[26] Low-performing students may defer taking one or more of the MCAS tests to their junior year to increase the likelihood that they will be able achieve a passing score.

[27] Note that the sample size differs by outcome depending on the time horizon (e.g., one year post or ever) and whether youth would be eligible to drop (e.g., given their age) or graduate (e.g., given their grade). See tables A13 and A14 for the point estimates from equation (1). Also note that at this point we do not have sufficient sample size to assess the impact of multiple summers of participation on dropout and high school graduation. However, future work may involve studying multiple cohorts which would provide a larger number of students to observe across multiple states of participation for these outcomes.

to ask whether SYEPs might have a disproportionate effect on subgroups—particularly those that are more likely to experience chronic absenteeism, as that is where the strongest program impacts have manifested. For example, prior research indicates that chronic absenteeism is more likely to be observed among older students, those with limited English ability, and at-risk youth such as those who are homeless or living in households that receive public assistance (Utah Education Policy Center 2012). We note that our subgroup analyses were not pre-specified, but rather, are exploratory. Still, exploratory subgroup analyses can be useful for generating new hypotheses and for robustness checking.

Table 6 reports the ITT estimate of the differential program impact on the improvement in academic outcomes for the subgroups listed in the Utah study as well as for "marginal" students—defined as those having either chronically high absenteeism or low GPAs (depending on the outcome of interest) during the baseline pre-period (e.g., the 2014-15 school year).[28] Among attendance outcomes, the Boston SYEP had a greater impact on students with prior chronic absenteeism as well as youth of legal drop-out age (e.g., 16 years or older)—both groups experience an additional 4 percentage point boost to their attendance rates compared to the average student in the treatment group. In terms of course performance, the program appears to have a disproportionate impact on improving overall GPA and reducing course failures among older students (e.g., age 16 years or older) as well as those with limited English ability. The latter finding is consistent with prior research that shows learning English is more effective in a contextualized setting, such as on the job (Burt and Mathews-Aydinli 2007). We found no differential impacts of the Boston SYEP on dropout or high school graduation for any of our

[28] For attendance, dropout, and graduation outcomes, marginal youth are defined as those who previously had an attendance rate below 90 percent in the year prior to SYEP participation (e.g., 2014-15 academic year). For course performance outcomes, marginal students are defined as those previously having an overall GPA that was below a C average in the year prior to SYEP participation.

subgroups, suggesting that this aspect of the program is more universal in nature.

What might be driving these results? It could be the case that participating in the SYEP improves behaviors that are correlated with academic success. For example, focus group participants repeatedly stressed that "being on time" is one of the most important lessons they learned at their summer job. It could also be the case that the program's career readiness curriculum, coupled with real-world experience, boosts career and academic aspirations that lead to greater motivation or effort in school during the following year. Finally, prior research has shown that SYEP reduce the propensity to engage in delinquent behavior, including truancy, that would be disruptive to learning. We explore these mechanisms further in the next section by assessing the degree to which SYEP participants learn new skills over the summer and how these changes are correlated with improvements in attendance after participating in the program.

B. Exploring SYEP Program Mechanisms Using Survey Data

1. Assessing Short-Term Behavioral Impacts

The self-reported survey data indicate that youth participating in the Boston SYEP experienced significant improvements across a variety of short-term behaviors and skills that could plausibly be correlated with the subsequent improvements in attendance, course performance, and dropout/graduation rates observed in the administrative data. Table 7 shows the change over time for the pre-/post-program survey responses of the treatment group as well as the difference between the post-program responses for the treatment versus the control group, estimated using equation (3).

Recall that we measure program impacts as those outcomes where there was a significant improvement among participants over the summer as well as a significant difference relative to the control group at the end of the summer. For example, panel A shows that the share of

participants reporting that they plan to attend a four-year college or university increased significantly by nearly 5 percentage points during the summer and was 11 percentage points higher than the share of the control group reporting similar academic aspirations at the end of the summer. Coincidentally, the share of SYEP participants who reported saving for college also increased by 5 percentage points and was significantly higher than that of the control group at the end of the summer. In contrast, although the share of participants reporting that they wanted to work in the fall increased by 7 percentage points, this measure was below that reported by the control group at the end of the summer.[29]

SYEP participants also indicated sizeable growth in job readiness skills during the summer, many of which were significantly greater those reported by the control group (see panel B of Table 7). This included large increases in the share of participants reporting that they had prepared a resume and a cover letter, practiced interviewing skills with an adult, and developed answers to typical interview questions. Perhaps more directly relevant to our earlier findings regarding school attendance is the significant increase in the share of participants who reported knowing "how to be on time" and "how to organize my work and keep to my schedule."

Finally, panel C of Table 7 indicates that participants' attitudes toward their communities improved greatly (by 15 percentage points), and these outcomes were significantly better than those reported by the control group at the end of the summer. Given that most SYEP job placements are with community-based organizations in the participants' neighborhoods, it could be that the program provides youth with an opportunity for more positive social engagement within their communities. Although smaller in magnitude, participants also showed significant

[29] If we think that youth might substitute working in the summer for time spent working in the fall, then this finding would be consistent with youth in the treatment group having a higher propensity to do so relative to the control group, most of whom did not work during the summer of 2015.

improvements in social skills and behaviors that have been shown to be correlated with academic success—such as managing emotions, asking for help, and resolving conflict with a peer—measures that were also significantly higher relative to the control group by the end of the summer. These improvements might reflect additional soft-skills development stemming from the program's career readiness curriculum and practiced on the job throughout the summer.

2. Evaluation of Program Mechanisms

Although participants demonstrated significant gains in a variety of short-term behaviors and skills according to the survey data, only some of those changes appear to be correlated with subsequent improvements in school outcomes. Table 8 reports the results of our mediation analysis from equation (4) that provides the ITT estimate of the program's impact on attendance while separately controlling for improvements in each of the short-term behavioral measures (ΔM_i) that showed significant improvements over the summer as well as relative to the control group. For example, Panel A reports the impact of measures related to academic aspirations and reveals that youth who reported that they had started to save for college over the summer experienced greater improvements in attendance and graduating on time. This finding suggests that the program not only operates through raising academic aspirations but also by providing youth with the knowledge and/or resources to act on those aspirations.

Panel B of Table 8 shows that improvements in work habits such as being on time and organizing one's work / keeping to a schedule were found to have positive impacts across all of our attendance measures, suggesting that the old adage that "80 percent of success is just showing up" might in fact be true.[30] Interestingly, improvements in almost all of the job readiness skills were significantly correlated with reductions in unexcused absences. Again, this

[30] In 1989 Woody Allen was asked about this saying by William Safire, the language columnist for the New York Times, and Allen replied with a letter in which he asserted: "I did say that 80 percent of success is showing up."

is suggestive of a behavioral shift as absences related to truancy are more likely to reflect choices made by youth, rather than other absences that might be related to illness.

Finally, improvements in social skills—such as managing emotions and asking for help—were almost universally correlated with increasing the likelihood of graduating from high school on time (see panel C of Table 8). In addition, gaining a mentor appears to have an impact on both high school graduation as well as reducing the truancy. These findings are consistent with prior research on summer jobs programs that has linked improvements in social skills to reductions in a wide range of delinquent and criminal behavior among youth (Modestino 2019).

Although these findings are only suggestive, the results presented here regarding the program's behavioral mechanisms are consistent with prior research on the effects of work-based learning programs in some high schools. These programs link classroom instruction to workplace skills through work-related outside placements such as internships, mentoring, workplace simulations, and apprenticeships. One study found that students in work-based learning programs complete related coursework at high rates and have higher attendance and graduation rates than those not enrolled in such programs (Colley and Jamison, 1998). Yet we note that our mediation analysis cannot fully disentangle the SYEP program effects from other factors—in particular, the benefits of simply providing youth and their families with additional income.

IV. Conclusion

Overall, we find that the Boston SYEP had a significant and meaningful impact on improving attendance, reducing dropout, and increasing graduation rates among youth—and a smaller but significant impact on course performance. During the school year after participation, youth who were randomly selected into the SYEP treatment group experienced significant improvements in attendance rates of 1.9 percentage points, in part by significantly reducing their

unexcused absences (-1.1 days). Moreover, youth in the treatment group were 7.8 percentage points more likely to achieve an attendance rate of 90 percent or better, reducing chronic absenteeism by 27 percent relative to the baseline. Larger improvements were found for youth with initially low attendance rates and those age 16 and older who were able to legally drop out of school. We also find small but significant impacts on course performance in the year after participation, but no meaningful improvements in rates of standardized test-taking or scores.

Looking at the two-year impacts suggests that the program's effects on attendance tend to fade out over time without a second summer of participation. Unlike youth who participated for only one summer, those that applied and were randomly selected to participate for a second summer appear to maintain the 1.9 percent improvement in their attendance rates from the first year. Similarly, the impact of the program on course performance appears to be even larger during the second year after participation—increasing GPAs and reducing course failures by 6 and 10 percentage points relative to baseline respectively—but only for those who participate for a second summer. Again, we need to be careful in attributing a causal interpretation to the second-year results for repeat participants given that youth need to have applied for a second time, possibly indicating greater intrinsic motivation or ability. Additional work is needed to more precisely estimate the minimum "dosage" (e.g., number of summers) needed to achieve meaningful impacts. This is a priority for currently oversubscribed programs, such as Boston, where participation is currently assigned by lottery. Currently, about one-third of the Boston SYEP funding comes from state sources, which stipulate that only 20 percent of the youth served in any given year can be repeat participants. Such participation constraints might not be efficient if it is indeed the case that multiple summers are needed to obtain lasting impacts.

Most importantly, we find that participating in the summer jobs program significantly

reduces the likelihood of dropping out of high school and correspondingly raises the likelihood of graduating. Being randomly selected into the Boston SYEP reduces the likelihood of dropout by 2.6 percentage points—or 24.8 percent—relative to the control group. Prior studies have documented that high school graduates have better outcomes than dropouts along a number of dimensions including higher employment rates and incomes (Child Trends 2017) as well as lower rates of criminal activity and take-up of social services (Lochner and Moretti 2001). By some estimates, each new high school graduate confers a net benefit to taxpayers of about $127,000 over the graduate's lifetime.[31] According to the City of Boston, the SYEP costs roughly $2,000 per participant, resulting in a total cost of $2.4 million for the 1,200 youth that participated through ABCD during the summer of 2015.[32] Given that the program appears to increase the likelihood of high school graduation by 6 percentage points, this would yield an additional 72 graduates, who on net would collectively confer a benefit of $9.1 million over their lifetimes, resulting in a benefit-to-cost ratio of 4-to-1.

However, it is not clear how the Boston SYEP compares with other interventions that do not involve the added direct costs of subsidized wages as well as indirect program administration costs of soliciting commitments from employers, matching teens to jobs at the start of each summer, and supervising youth at multiple job sites. For example, other studies have found that lower-cost interventions, such as notifying parents of absences via postcard (10 percent) or text messaging (-17 percent), produce improvements in attendance rates that are similar in magnitude (Rogers and Feller 2014) to those we found for the Boston SYEP. Yet, SYEPs also provide

[31] Levin, Henry and Cecelia Rouse. 2012. "The True Cost of High School Dropouts." The New York Times, January 25, 2012. https://www.nytimes.com/2012/01/26/opinion/the-true-cost-of-high-school-dropouts.html

[32] This includes an average of just over $1,400 in wages. From a societal perspective, the wage cost is simply a transfer from the government to the youth and so is not generally counted as a net change in overall resources. This leaves an administrative program cost of $600, although if one wanted to separate the costs and benefits that accrue to the government, participants, and society, then wages would appear as a cost to the government and a benefit to participants.

additional benefits to individuals and their families that may also outweigh the program's costs. For example, SYEPs confer job experience, which may yield additional advantages in terms of future employment, career pathways, or post-secondary education. In addition, SYEPs help families at or near the poverty line by providing income to youth—with upwards of one in five youth contributing directly to their household's expenses, according to our survey data—potentially increasing household resources that can affect a wide range of youth outcomes.

Finally, by linking the academic records to self-reported survey data on short-term changes in behaviors and skills, we are able to shed light on how the program achieves these better outcomes among the youth being served. Our mediation analysis reveals that the program develops basic work habits, increases aspirations to attend college, and improves social skills—and that these behavioral changes are correlated with subsequent improvements in attendance as well as the likelihood of graduating from high school on time. These findings give researchers some insights into the behavioral changes that occur during the program while also providing a look inside the "black box" as to how SYEPs affect youth outcomes in the long run.

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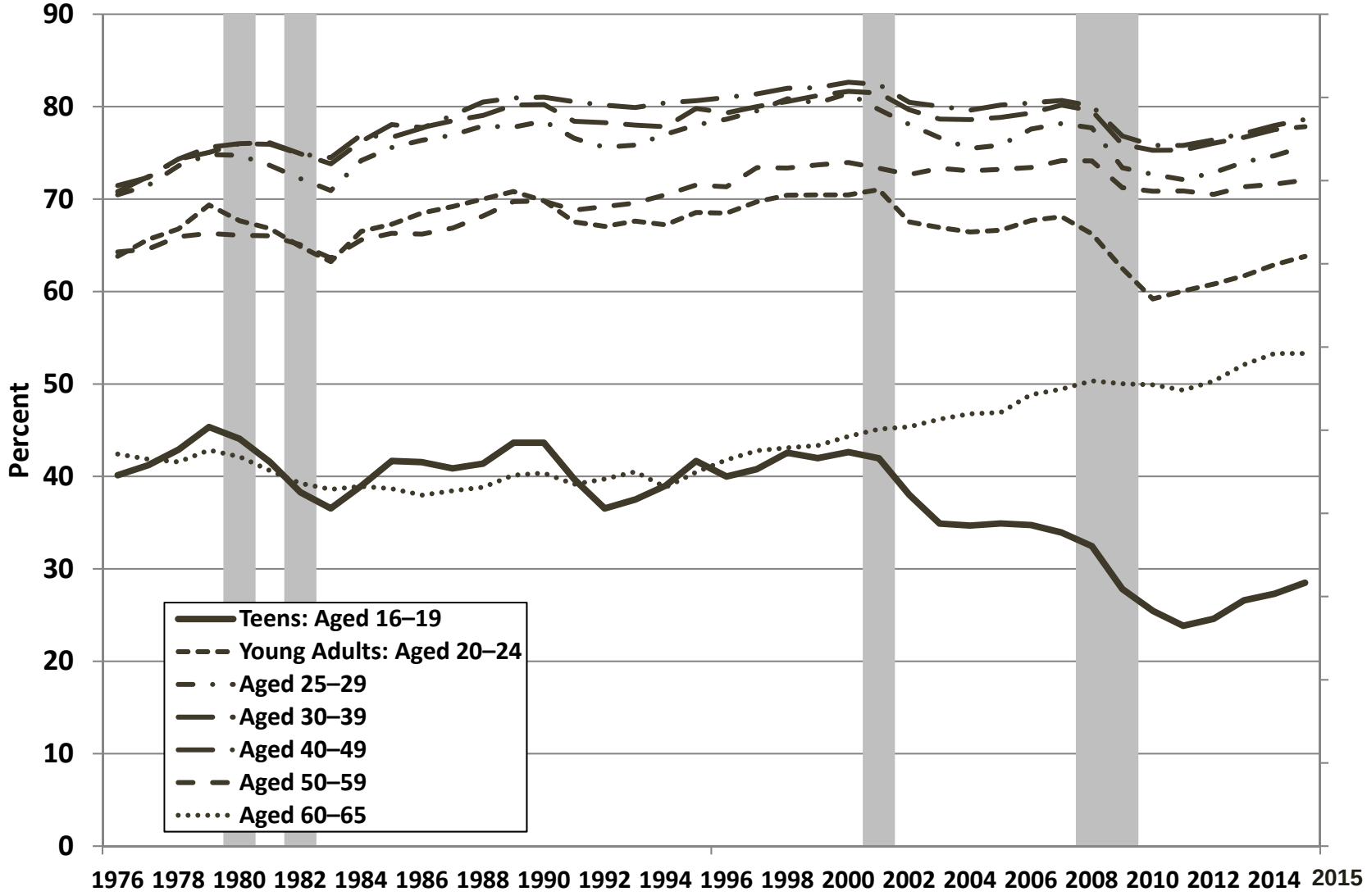
FIGURE 1. U.S. EMPLOYMENT-TO-POPULATION RATIO BY AGE GROUP, 1976–2015.

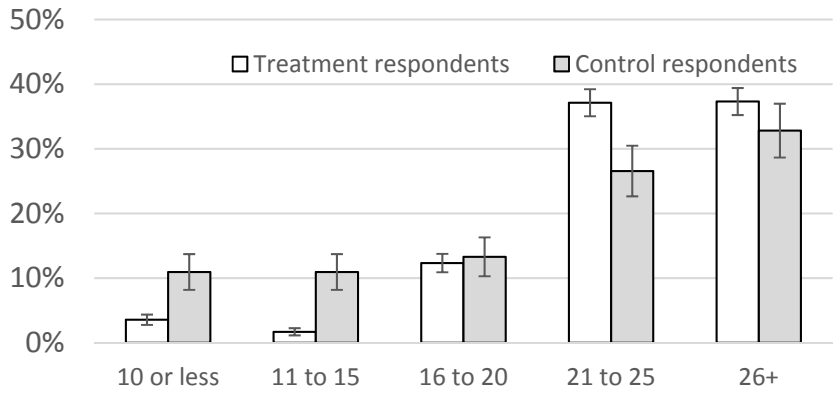


Note: The figure shows the employment to population ratio has declined more steeply for teens aged 16-19 relative to other age groups. Shaded bars represent recession periods as defined by the National Bureau of Economic Research.

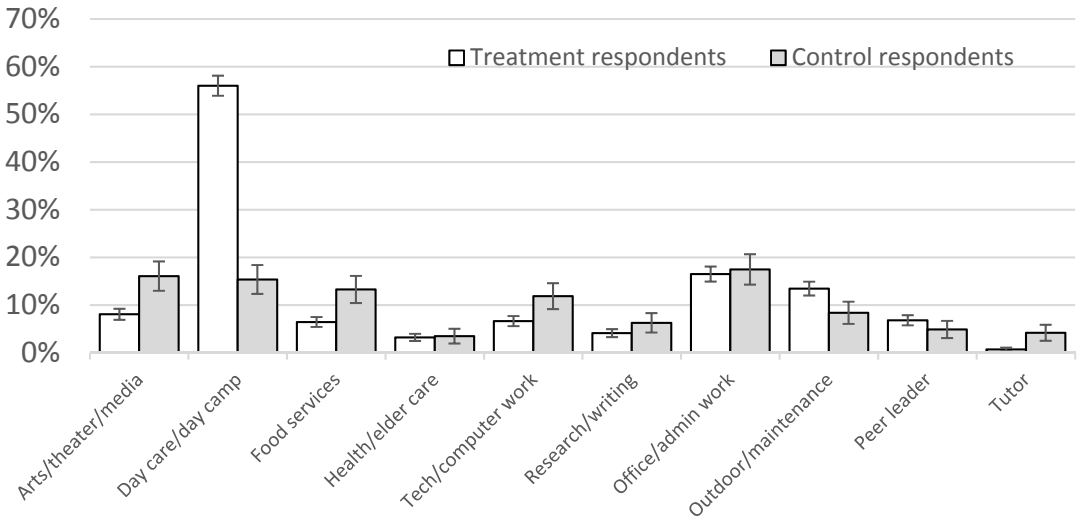
Source: Author's calculations from the U.S. Census Bureau, Current Population Survey, various years.

FIGURE 2. SUMMER EXPERIENCES FOR SYEP SURVEY RESPONDENTS BY LOTTERY OUTCOME.

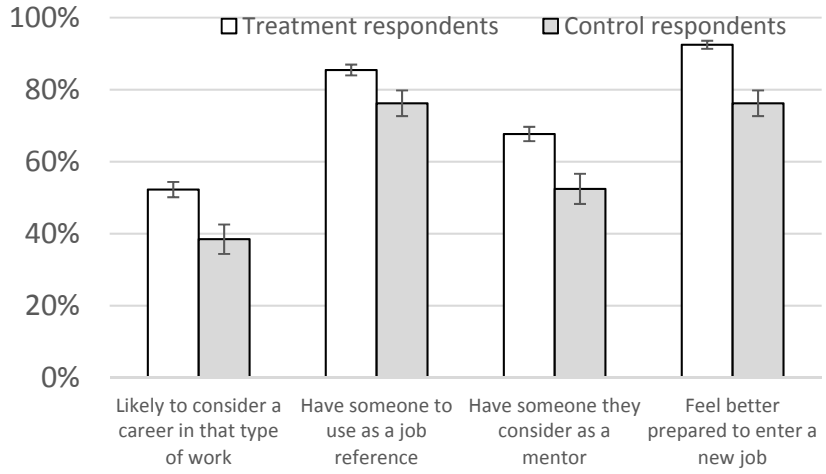
A. Hours worked per week.



B. Type of daily work.



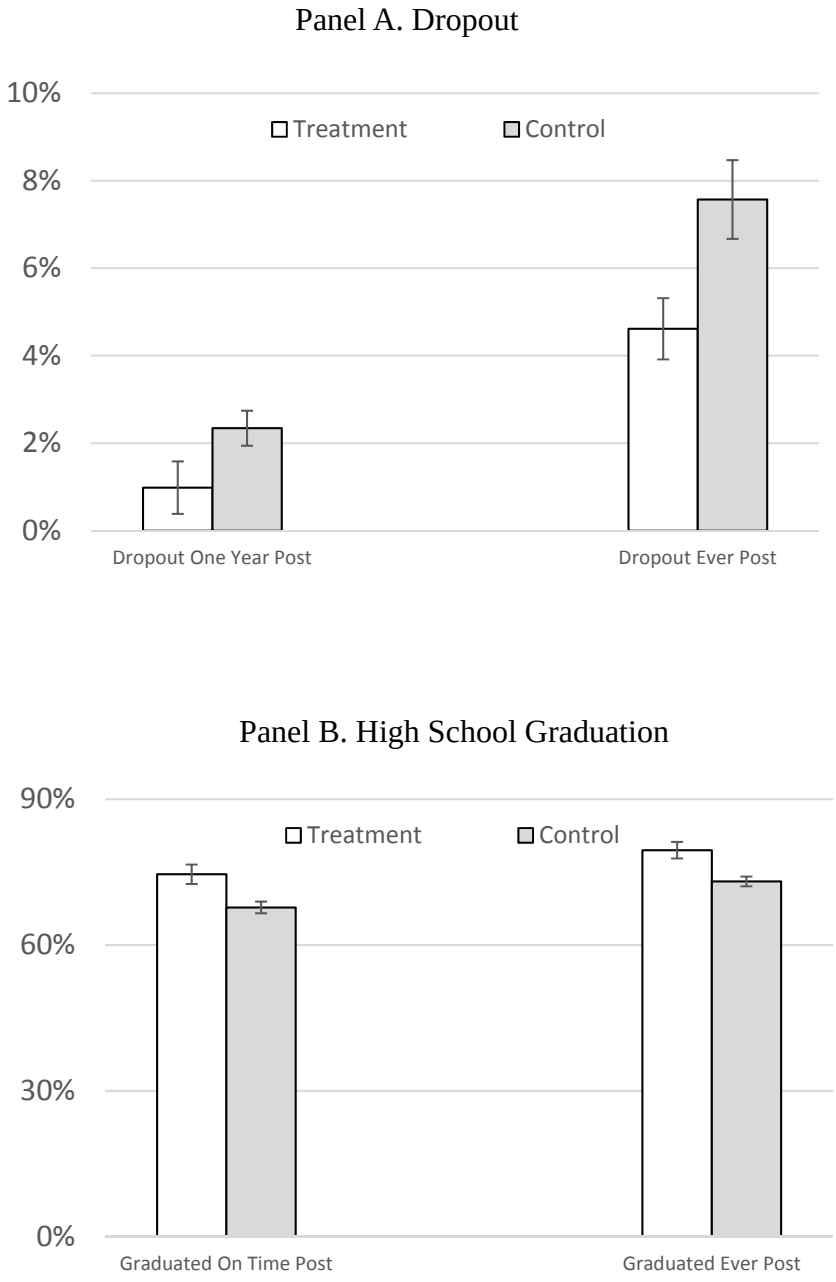
C. Satisfaction with job experience.



Notes: This figure displays descriptive information about the self-reported summer employment experiences among individuals responding to an end-of-summer survey of both the treatment group and control groups to assess whether the Boston SYEP provides a meaningful intervention. Individuals in the treatment group work more hours per week, are more likely to work in day cares and day camps, and are more satisfied with their job experience.

Source: Survey data provided by the City of Boston, Office of Workforce Development.

FIGURE 3. ITT PROGRAM IMPACTS ON DROPOUT AND HIGH SCHOOL GRADUATION.



Notes: This figure presents estimates of the program's impact on both dropout and high school graduation. The sample includes youth who were matched in both the 2014-15 and 2015-16 school years. Each coefficient is from a separate probit regression where the dependent variable is the likelihood of the outcome listed and the controls include SYEP participation through another intermediary, demographic characteristics (age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status), academic characteristics (grade level, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years), and school fixed effects. The coefficients reported in the table are the marginal effects, estimated at means.

Source: Administrative data on program participation provided by the City of Boston Office of Workforce Development. Administrative data from school records provided by the Massachusetts Department of Elementary and Secondary Education.

TABLE 1—SYEP APPLICANT CHARACTERISTICS BY LOTTERY OUTCOME

	Selected (treatments)		Not Selected (controls)		Treatment-Control	
	Mean	Std. Error	Mean	Std. Error	Difference	*p* -value
Age	15.917	(0.058)	15.845	(0.033)	0.073	(0.258)
Percentage 14-17 years	0.794	(0.008)	0.802	(0.007)	-0.008	(0.292)
Percentage female	0.531	(0.014)	0.539	(0.009)	-0.008	(0.640)
Percentage in school	0.876	(0.010)	0.884	(0.006)	-0.008	(0.497)
Percentage African American	0.513	(0.015)	0.540	(0.009)	-0.027	(0.197)
Percentage Asian	0.065	(0.007)	0.050	(0.004)	0.015	(0.088)
Percentage White	0.096	(0.009)	0.084	(0.005)	0.012	(0.211)
Percentage other/two or more races	0.325	(0.014)	0.326	(0.009)	0.000	(0.983)
Percentage Chinese	0.002	(0.001)	0.001	(0.001)	0.001	(0.557)
Percentage English	0.951	(0.006)	0.955	(0.004)	-0.004	(0.620)
Percentage Spanish	0.033	(0.005)	0.027	(0.003)	0.006	(0.287)
Percentage other language	0.014	(0.003)	0.018	(0.002)	-0.003	(0.465)
Percentage limited English ability	0.071	(0.007)	0.071	(0.005)	0.000	(0.969)
Percentage homeless	0.067	(0.007)	0.069	(0.005)	-0.002	(0.822)
Percentage receiving public assistance	0.187	(0.011)	0.172	(0.007)	0.015	(0.240)
Percentage disabled	0.040	(0.006)	0.033	(0.003)	0.007	(0.276)
Number of youth	1,186		3,049		-1,863	

Notes: The table shows that the treatment variable is uncorrelated with the individual's background variables. Each line of the table provides the mean of the the background variable listed in the first column for the treatment versus the control group as well as the difference between the two groups. The last column provides the p-value from a regression of the background variable on the treatment dummy. The only statistically significant difference is the share of Asian youth being slightly higher (7 percent) in the treatment group versus the control group (5 percent). Having at least one statistically significant difference at the $p<0.10$ level would be expected by random chance when testing 15 different characteristics.

Source: Author's calculations based on application data provided by the City of Boston Office of Workforce Development.

TABLE 2—TESTING FOR DIFFERENTIAL ATTRITION FROM ADMINISTRATIVE DATA BY LOTTERY OUTCOME

	Applicants matched to DESE records during school year prior to participation (2014-15)		Applicants matched to DESE records for both pre (2014-15) and post (2015-16) school years	
	Effect of winning the lottery	*p* -vlaue	Effect of winning the lottery	*p* -vlaue
Age	-0.000	0.998	-0.007	0.298
Male	0.026	0.180	0.026	0.126
Black	-0.011	0.541	-0.011	0.538
White	0.039	0.224	0.033	0.327
Asian	0.071	0.064	0.082	0.039
Language Chinese	0.069	0.765	0.207	0.424
Language English	0.017	0.762	0.042	0.475
Language Spanish	0.058	0.438	0.114	0.146
Limited English Ability	-0.006	0.855	-0.016	0.645
Homeless	-0.016	0.635	-0.024	0.510
Public Assistance	0.029	0.180	0.030	0.178
Disability	-0.007	0.875	-0.021	0.670
Number of youth	3,269		2,970	
F-value, test of joint significance	1.08		1.40	

Notes: This table tests for differential attrition across the treatment and control groups by comparing estimates of the effect of winning the lottery on preexisting demographic characteristics for the sample to youth who were matched in the 2014-15 school year versus the sample to those who were also enrolled in the 2015-16 school year. The dependent variable is a binary variable which takes on a value of 1 if the individual participated in the SYEP. The SYEP indicator does not significantly predict any individual characteristics—with the exception of the one characteristic (e.g. Asian) that was noted in the earlier balance test—suggesting that overall SYEP lottery winners and losers did not differentially attrit.

Source: Adminsitrative data on program participation was provided by the Boston Mayor's Office of Workforce Development (OWD). Adminsistrative data on school records was provie by the Massachusetts Department of Elementary and Secondary Education (DESE).

TABLE 3—ITT ESTIMATES OF PROGRAM IMPACT ON ATTENDANCE

	One Year Post						Two Years Post	
	Coefficient on Treatment Dummy						Coefficient on:	
							One summer of treatment	Two summers of treatment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Attendance rate	0.025	0.027	0.028	0.031	0.018	0.019	0.010	0.019
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.011)
Increased attendance rate	0.048	0.039	0.042	0.039	0.039	-----	0.042	-0.035
	(0.028)	(0.028)	(0.028)	(0.028)	(0.021)		(0.023)	-0.032
Decreased attendance rate	-0.067	-0.057	-0.060	-0.056	-0.056	-----	-0.045	0.024
	(0.028)	(0.029)	(0.029)	(0.029)	(0.021)		(0.023)	(0.033)
Attendance rate >=90%	0.066	0.072	0.074	0.081	0.061	0.078	0.005	0.031
	(0.026)	(0.027)	(0.026)	(0.028)	(0.018)	(0.006)	(0.022)	(0.032)
Average days attended	3.604	4.323	4.464	5.208	3.204	3.089	1.583	4.660
	(2.044)	(2.100)	(2.046)	(2.003)	(1.335)	(0.006)	(1.507)	(2.282)
Unexcused absences	-2.514	-2.639	-2.594	-2.648	-1.754	-1.196	-1.441	-2.753
	(1.168)	(1.185)	(1.064)	(0.980)	(0.058)	(0.006)	(1.223)	(1.481)
Other SYEP participation	No	Yes	Yes	Yes	Yes	Yes	Yes	
Demographic characteristics	No	No	Yes	Yes	Yes	Yes	Yes	
Academic characteristics	No	No	No	Yes	Yes	Yes	Yes	
School fixed effects	No	No	No	No	Yes	Yes	Yes	
Baseline outcomes	No	No	No	No	No	Yes	Yes	
Number of youth	2,852	2,852	2,852	2,852	2,852	2,852	2,439	

Notes: This table estimates the impact of SYEP participation on attendance related outcomes. The sample for specifications (1)-(6) includes youth who were matched in 2014-15 and 2015-16. Specification (7) includes youth who were matched in both 2014-15 and 2016-17. For specifications (1)-(6), each coefficient is from a separate regression where the dependent variable is the outcome listed. For specification (7), coefficients on indicators for having participated in one summer or in two summers are given from the same regression where the dependent variable is the outcome listed. Other SYEP participation controls for whether youth in the control group participated in the SYEP through another intermediary. Demographic characteristics include age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status. Academic characteristics include indicators for grade, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years. Probit is used to estimate results for binary outcomes such as increasing or decreasing attendance rate. A Poisson specification is used to estimate the impact on days attended and days truant. For these non-linear specification, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors are in parentheses.

Source: Adminsitrative data on program participation was provided by the Boston Mayor's Office of Workforce Development (OWD). Adminsistrative data on school records was provie by the Massachusetts Department of Elementary and Secondary Education (DESE).

TABLE 4—ITT ESTIMATES OF PROGRAM IMPACT ON COURSE PERFORMANCE

	One Year Post						Two Years Post	
	Coefficient on Treatment Dummy						Coefficient on:	
							One summer of treatment	Two summers of treatment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Overall GPA	0.085	0.102	0.085	0.080	0.113	0.080	0.037	0.123
	(0.050)	(0.051)	(0.049)	(0.046)	(0.042)	(0.036)	(0.049)	(0.074)
Percentage failing any course	0.001	-0.006	-0.007	-0.007	-0.035	-0.023	0.029	-0.061
	(0.031)	(0.031)	(0.032)	(0.033)	(0.021)	(0.021)	(0.023)	(0.035)
Percentage failing a math course	-0.011	-0.013	-0.007	-0.004	-0.028	-0.018	0.041	-0.100
	(0.031)	(0.032)	(0.032)	(0.033)	(0.022)	(0.021)	(0.086)	(0.128)
Percentage failing an ELA course	0.006	0.006	0.007	0.012	-0.016	-0.008	-0.020	-0.102
	(0.031)	(0.032)	(0.032)	(0.033)	(0.021)	(0.021)	(0.024)	(0.038)
Other SYEP participation	No	Yes	Yes	Yes	Yes	Yes	Yes	
Demographic characteristics	No	No	Yes	Yes	Yes	Yes	Yes	
Academic characteristics	No	No	No	Yes	Yes	Yes	Yes	
School fixed effects	No	No	No	No	Yes	Yes	Yes	
Baseline outcomes	No	No	No	No	No	Yes	Yes	
Number of youth	2,327	2,327	2,327	2,327	2,327	2,327	1,727	

Notes: This table estimates the impact of SYEP participation on course performance. The sample for specifications (1)-(6) includes youth who were matched in 2014-15 and 2015-16. Specification (7) includes youth who were matched in both 2014-15 and 2016-17. For specifications (1)-(6), each coefficient is from a separate regression where the dependent variable is the outcome listed. For specification (7), coefficients on indicators for having participated in one summer or in two summers are given from the same regression where the dependent variable is the outcome listed. Other SYEP participation controls for whether youth in the control group participated in the SYEP through another intermediary. Demographic characteristics include age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status. Academic characteristics include indicators for grade, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years. Probit is used to estimate results for binary outcomes. A Poisson specification is used to estimate the impact on days attended and days truant. For these non-linear specification, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors are in parentheses.

Source: Adminsitrative data on program participation was provided by the Boston Mayor's Office of Workforce Development (OWD). Adminsistrative data on school records was provie by the Massachusetts Department of Elementary and Secondary Education (DESE).

TABLE 5—ITT ESTIMATES OF PROGRAM IMPACT ON STANDARDIZED TEST-TAKING AND PERFORMANCE

	Coefficient on Treatment Dummy				
	(1)	(2)	(3)	(4)	(5)
Panel A. Mathematics					
Took MCAS on time	0.031	0.048	0.045	0.039	0.025
	(0.039)	(0.039)	(0.042)	(0.042)	(0.026)
Scaled score	0.583	0.290	0.359	0.712	0.764
	(1.321)	(1.379)	(1.234)	(1.220)	(1.104)
Percentage proficient or better	-0.011	-0.003	0.014	0.024	0.028
	(0.049)	(0.050)	(0.052)	(0.052)	(0.039)
Number of youth	803	803	803	803	803
Panel B. English Language Arts					
Took MCAS on time	0.053	0.069	0.064	0.057	0.036
	(0.037)	(0.037)	(0.037)	(0.036)	(0.026)
Scaled score	-0.559	-0.377	-0.019	0.253	0.553
	(0.844)	(0.895)	(0.794)	(0.780)	(0.765)
Percentage proficient or better	-0.031	-0.018	-0.012	-0.005	0.003
	(0.035)	(0.035)	(0.033)	(0.032)	(0.039)
Number of youth	815	815	815	815	815
Other SYEP participation	No	Yes	Yes	Yes	Yes
Demographic characteristics	No	No	Yes	Yes	Yes
Academic characteristics	No	No	No	Yes	Yes
School fixed effects	No	No	No	No	Yes

Notes: This table tests for the impact of SYEP participation on standardized test taking and performance. The sample includes youth who were matched in 2014-15 and 2015-16 and were in grade 9 in the 2014-15 school year. Test-taking is assessed for all youth who were in grade 9 in the 2014-15 school year (N= 1,029 youth). Performance is assessed for youth who took the exam. Each coefficient is from a separate regression where the dependent variable is the outcome listed. Other SYEP participation controls for whether youth in the control group participated in the SYEP through another intermediary. Demographic characteristics include age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status. Academic characteristics include indicators for grade, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years. Probit is used to estimate results for binary outcomes. For these non-linear specification, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors are in parentheses.

Source: Adminsitrative data on program participation was provided by the Boston Mayor's Office of Workforce Development (OWD). Adminsistrative data on school records was provie by the Massachusetts Department of Elementary and Secondary Education (DESE).

TABLE 6—ITT ESTIMATES OF PROGRAM IMPACT ON ONE-YEAR ACADEMIC OUTCOMES BY SUBGROUP

	Coefficient on Treatment Dummy* Group Dummy					Total number of youth in each regression
	Marginal Students	Age 16+	Male	Limited English	Public Assistance	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Attendance Outcomes						
Attendance rate	0.037	0.042	0.008	-0.004	-0.006	2,852
	(0.016)	(0.013)	(0.012)	(0.018)	(0.016)	
Increased attendance rate	0.050	0.086	0.015	-0.016	-0.080	2,852
	(0.053)	(0.050)	(0.047)	(0.080)	(0.056)	
Decreased attendance rate	-0.031	-0.099	-0.022	0.127	0.078	2,852
	(0.054)	(0.050)	(0.048)	(0.081)	(0.059)	
Attendance rate >=90%	0.065	0.109	0.043	-0.078	-0.052	2,852
	(0.049)	(0.046)	(0.049)	(0.102)	(0.066)	
Average days attended	8.146	5.039	0.847	-1.798	-1.878	2,852
	(4.329)	(3.429)	(3.056)	(5.584)	(3.796)	
Unexcused absences	0.567	-1.864	-0.339	0.056	3.135	2,852
	(1.391)	(1.504)	(1.532)	(2.503)	(2.345)	
Panel B. Course Performance						
Overall GPA	0.099	0.146	-0.054	0.136	0.048	2,327
	(0.071)	(0.080)	(0.071)	(0.133)	(0.097)	
Percentage failing any course	-0.096	-0.048	0.044	-0.224	0.001	2,327
	(0.057)	(0.064)	(0.054)	(0.108)	(0.074)	
Percentage failing a math course	-0.058	-0.059	-0.010	0.020	0.033	2,327
	(0.054)	(0.060)	(0.055)	(0.104)	(0.073)	
Percentage failing an ELA course	-0.053	-0.153	0.042	-0.130	0.027	2,327
	(0.055)	(0.057)	(0.056)	(0.092)	(0.073)	
Panel C. Dropout						
Dropped out post	-0.005	0.002	-0.001	0.006	0.002	2,970
	(0.009)	(0.013)	(0.012)	(0.027)	(0.016)	
Dropped out ever	-0.004	-0.004	0.004	-0.007	0.018	2,970
	(0.021)	(0.024)	(0.025)	(0.043)	(0.035)	
Panel D. Graduation						
Graduated on time	0.033	0.022	0.013	0.014	-0.047	1,953
	(0.062)	(0.060)	(0.060)	(0.102)	(0.083)	
Graduated ever	-0.005	0.047	0.017	0.037	-0.005	1,953
	(0.060)	(0.064)	(0.056)	(0.090)	(0.077)	
Number of youth in subgroup	852	1,145	1,346	207	514	-----

Notes: This table tests for differential impacts of SYEP participation on education outcomes for various subgroups. The sample includes youth who were matched in 2014-15 and 2015-16. Each coefficient is from a separate regression for the listed outcome and all regressions include the SYEP treatment dummy as well as the interaction of the treatment dummy with the group-level dummy. For attendance, dropout, and graduation outcomes, marginal youth are defined as those who previously had an attendance rate below 90 percent in the year prior to SYEP participation (e.g., 2014-15 academic year). For course performance outcomes, marginal students are defined as those previously having an overall GPA that was below a C average in the year prior to SYEP participation. Each regression includes the full set of covariates from the previous tables including whether youth in the control group participated in the SYEP through another intermediary, demographic characteristics (age, gender, race, primary lagnuage spoken, limited English, public assistance, homelessness, and disabled status), academic characteristics (e.g., grade level, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years). Probit is used to estimate results for binary outcomes. Poisson regressions are used to estimate results for count outcomes. Coefficients reported in the table from non-linear estimation are marginal effects, estimated at means. Robust standard errors are in parentheses.

Source: Adminsitrative data on program participation was provided by the Boston Mayor's Office of Workforce Development (OWD). Adminsistrative data on school records was provie by the Massachusetts Department of Elementary and Secondary Education (DESE).

TABLE 7—ASSESSING SHORT-TERM BEHAVIORAL CHANGES IN SKILLS AND ATTITUDES

	Treatment				Treatment-Control	
	(1)	(2)	(3)		(4)	
	Pre-program	Post-program	Post-Pre		Post	
	Mean	Mean	Difference	SE	Difference	SE
Future work plans and academic aspirations						
I plan to work in the fall	0.406	0.480	0.074	0.009	-0.074	0.030
I plan to enroll in an eduation or training program after high school	0.674	0.703	0.029	0.014	0.003	0.017
I plan to enroll in a four-year college or university	0.681	0.730	0.049	0.019	0.110	0.029
I plan to enroll in a two-year college	0.129	0.124	-0.005	0.015	0.062	0.019
I am saving for school tuition	0.062	0.114	0.052	0.012	0.043	0.021
Job readiness skills						
I have all key information to apply for a job	0.810	0.882	0.072	0.021	0.094	0.021
I have prepared a resume	0.409	0.701	0.293	0.033	0.245	0.027
I have prepared a cover letter	0.234	0.437	0.204	0.039	0.217	0.027
I have asked an adult to serve as a reference	0.709	0.745	0.036	0.026	-0.001	0.027
I have reviewed at least one job application form	0.748	0.824	0.075	0.023	0.039	0.028
I have completed at least one online job application form	0.661	0.709	0.048	0.025	-0.033	0.028
I have searched for jobs online	0.477	0.596	0.119	0.030	0.025	0.031
I have asked an adult for help in finding job opportunities	0.830	0.846	0.017	0.020	0.071	0.024
I have developed answers to the usual interview questions	0.679	0.771	0.092	0.027	0.069	0.026
I have practiced my interviewing skills with an adult	0.548	0.649	0.101	0.021	0.064	0.031
I know how to be on time	0.431	0.540	0.110	0.018	0.081	0.015
I know how to organize my work and keep to my schedule	0.418	0.510	0.092	0.014	0.086	0.016
Community engagement and social skills						
I have a lot to contribute to the groups I belong to	0.319	0.466	0.147	0.023	0.156	0.029
I feel connected to people in my neighborhood	0.220	0.368	0.148	0.021	0.212	0.025
I feel safe walking around my neighborhood	0.429	0.467	0.038	0.022	0.193	0.028
I have a positive role model in my life	0.916	0.926	0.010	0.008	0.005	0.011
I know how to manage my emotions and my temper	0.442	0.497	0.055	0.023	0.065	0.033
I know how to ask for help when I need it	0.445	0.487	0.042	0.020	0.116	0.030
I have a mentor	0.476	0.677	0.201	0.019	0.152	0.024
I know how to constructively resolve a conflict with a peer	0.366	0.422	0.057	0.018	0.136	0.029
Number of youth	663	663	663		1,327	

Notes: This tables estimates the changes behaviors and attitudes over the summer for the treatment group as well as the end-of-summer responses for the treatement versus the control groups. Difference over time pre versus post is a simple comparison of means for the same sample of participants completing both surveys. Difference in post-program responses for participants versus controls is the marginal effect showing the difference in the predicted probabilities from a separate probit regression of the outcome on a dummy variable for treatment controlling for age, gender, race, two-parent family, and English as the primary language.

Source: S urvey data provided by the City of Boston Office of Workforce Development.

TABLE 8—RELATIONSHIP BETWEEN SHORT-TERM BEHAVIORAL CHANGES AND SYEP IMPACT ON ACADEMIC OUTCOMES: ITT ESTIMATES

	(1)		(2)		(3)		(4)		(5)	
	Attendance rate		Attendance rate>=90%		Unexcused absences		Dropped out ever		Graduated on time	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Panel A. Academic aspirations										
Planning to attend a four-year college	0.009	(0.012)	0.041	(0.950)	-1.980	(2.367)	0.007	(0.027)	-0.016	(0.051)
Saving for tuition	0.047	(0.025)	0.011	(0.102)	-10.454	(4.742)	-----		0.180	(0.100)
Panel B. Job readiness skills										
Having key information to apply for a job	-0.002	(0.014)	0.061	(0.038)	-2.144	(1.908)	0.029	(0.023)	-0.025	(0.046)
Preparing a resume	0.018	(0.010)	0.031	(0.032)	-3.515	(1.613)	0.000	(0.022)	0.046	(0.040)
Preparing a cover letter	0.018	(0.012)	0.050	(0.035)	-3.464	(1.820)	0.022	(0.022)	0.029	(0.043)
Developing answers to interview questions	-0.008	(0.014)	0.057	(0.036)	-1.943	(1.791)	0.026	(0.022)	0.000	(0.043)
Practicing interviewing with an adult	0.009	(0.011)	0.047	(0.035)	-1.806	(1.668)	0.002	(0.023)	0.029	(0.043)
Being on time	0.020	(0.009)	0.070	(0.031)	-2.720	(1.383)	-0.052	(0.023)	0.103	(0.037)
Keeping a schedule	0.025	(0.009)	0.087	(0.031)	-2.287	(1.382)	-0.029	(0.022)	0.062	(0.037)
Panel C. Community engagement and social skills										
Contributing to the groups they belong to	0.018	(0.011)	0.004	(0.041)	-2.871	(2.097)	-0.055	(0.032)	0.137	(0.052)
Connecting to people in their neighborhood	0.014	(0.013)	0.059	(0.044)	-3.287	(2.512)	-0.007	(0.030)	0.119	(0.058)
Managing emotions	0.020	(0.012)	-0.008	(0.051)	-2.390	(2.107)	-0.080	(0.046)	0.150	(0.059)
Asking for help	0.015	(0.011)	0.027	(0.049)	-4.342	(2.471)	-0.014	(0.032)	0.134	(0.057)
Gaining a mentor	0.016	(0.010)	0.015	(0.029)	-3.801	(1.369)	-0.026	(0.019)	0.099	(0.035)
Resolving conflict with a peer	0.003	(0.010)	-0.018	(0.043)	0.104	(1.769)	-0.004	(0.030)	0.045	(0.022)
Other SYEP participation	Yes		Yes		Yes		Yes		Yes	
Demographic characteristics	Yes		Yes		Yes		Yes		Yes	
Academic characteristics	Yes		Yes		Yes		Yes		Yes	
Baseline outcomes	Yes		Yes		Yes		Yes		Yes	
Number of youth	2,852		2,852		2,852		2,970		1,953	

Notes: This table estimates the relationship between improvements in short-term behaviors and skills that occur over the summer and subsequent imporvements in attendance during the year after participating in the program. The sample includes youth who were matched in 2014-15 and 2015-16. Other SYEP participation controls for whether youth in the control group participated in the SYEP through another intermediary. Demographic characteristics include age, gender, race, primary language spoken, limited English, public assistance, homelessness, and disabled status. Academic characteristics include indicators for grade, enrollment in a BPS school, high need special education status, participation in the METCO program, switching schools within the school year, and switching schools across school years. Probit is used to estimate results for binary outcomes. A Poisson specification is used to estimate the impact on days attended and days truant. For these non-linear specification, the coefficients reported in the table are the marginal effects, estimated at means. Robust standard errors are in parentheses.

Source: Adminsitrative data on program participation was provided by the Boston Mayor's Office of Workforce Development (OWD). Adminsistrative data on school records was provie by the Massachusetts Department of Elementary and Secondary Education (DESE).